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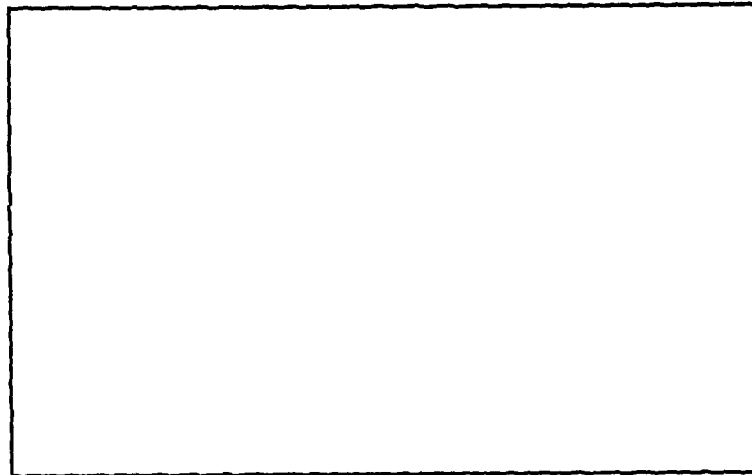
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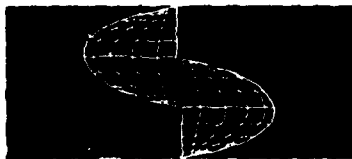
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EMPIRICAL EVIDENCE ON THE CAUSES OF
INTERNATIONAL VIOLENCE

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ABSTRACT

This paper attempts to collect the results of empirical studies on the causes and correlates of international violence. The results to date suggest that there are probably no single attributes of states that are responsible for international violence. On the other hand, there is evidence to the effect that certain combinations of attributes could make a state violence prone. In addition, it was found that there are environmental factors which appear to make violence more likely. The difficulty inherent in making comparisons over highly disparate studies, however, necessarily makes this overview inconclusive and it is, therefore, offered only as an initial and partial interpretation of existing empirical work.

EMPIRICAL EVIDENCE ON THE CAUSES OF
INTERNATIONAL VIOLENCE

Dina A. Zinnes
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This essay is a stock-taking exercise: what do we know about
international violence based on the quantitative research efforts of
the past thirty years? In one sense such an enterprise is self-
justifying: international violence is a major area of concern for the
field of international politics and an assessment of what we do and don't
know is certainly useful as a basis for suggesting what should be done
next. Although this is clearly one dimension of justification, there
is a second perhaps more important rationale for this review.

In a provocative article, Anatol Rapoport (1976) suggests that
theories or models of international politics -- and therefore of such
phenomena as international violence -- are not possible. Unlike physics,
argues Rapoport, the study of international political phenomena has
uncovered no laws upon which to build theories. By "law" Rapoport means
consistently observed regularities or patterns in the phenomena under
study, and he cites examples from our field to show the extent to which
our results appear ephemeral, transient and sometimes contradictory.
While Rapoport suggests other reasons for why international politics
can never be a science like physics, the apparent absence of "laws"
is the most serious charge.

Rapoport's criticism is a clear challenge to our field: Is it
the case that, after thirty years of empirical research in which we
have devoted enormous amount of time to collecting, measuring and

summarizing observations about nation-state behavior, we cannot find any patterns, that all of our results are disjoint bits and pieces which at best contradict one another and at worst appear to be totally disjoint? While one might not agree with Rapoport's premise that theories _must_ be built on laws, it is nevertheless the case that if we _could_ demonstrate the existence of such laws, it would be an invaluable asset in building theories.

Thus my second rationale for this essay is to accept Rapoport's challenge within the subject area of international violence: what regularities can we show after thirty years of research on violent nation-state conflict?

Procedure

Before we can begin, it is obvious that a few definitions are in order. How do we identify the relevant studies?

First, it is important to emphasize that the phenomenon under study is _inter-nation_ violence. Consequently, only studies involving _nation states_ are relevant (for example, Divale et. al.'s 1976 study of pre-industrial societies and other similar analyses are omitted here), and only those studies of nation states in which _violence_ between states is examined. There are a number of events-data studies which examine the general hostile behavior of nations. The only relevant portions of these studies for our purposes are those hostile behaviors that are specifically violent, i.e., that involve injury or death to the citizens of one state as a consequence of the actions of another state. For example, only a small portion of the work done by such researchers as Rummel, are of relevance, and the hostile interaction analyses of McClelland are not

considered. Furthermore, studies which combined general hostility and violence so that it was not possible to dissect out those analyses specific to violence, were also usually omitted; Pearson's (1974) analysis of intervention obviously includes some violent activities but the analyses of the violent interventions cannot be separately analyzed from interventions more generally, and most of Rummel's field theory work has similar problems. While some might feel that this narrowing of focus is unfortunate, I found it essential for the purposes of finding a reasonable starting point. Eventually we will want to go back and incorporate these other obviously relevant pieces of information, but to do so initially is to invite chaos.

Second, it is equally important to bear in mind that our concern here is with the outbreak of international violence. Our purpose is to attempt to discern whether there are any patterns that can be observed across studies which allow us to draw conclusions about why international violence occurs. Consequently, studies that deal with attributes of international violence, once underway, were not deemed relevant for this analysis. For example, studies of the conditions that determine victory in war (Rosen, 1972), characteristics of wars (Rummel, 1967, or Voevodsky, 1969), or the conditions under which wars will terminate (Klingberg, 1966), were omitted from consideration. Similarly, studies that analyzed the consequences of wars (Organski, 1977), were also omitted. Thus this analysis is very specifically focused on the outbreak of international violence.

Finally, it was felt that to adequately answer Rapoport's challenge we could only consider empirical studies. If we are to discern laws, they must be laws that have evolved from numerous observations, they

cannot be simple assertions even by eminent scholars in our field. Thus

the writings of political theorists, historians and even mathematical

modelers are not legitimate pieces of evidence unless they contain

careful and systematic observations, collected under replicable conditions.

In short, essays on why international violence occurs (e.g. the work of

Ken Waltz) are not considered.

The obvious starting point for a review such as this is Jones and

Singer (1972) and this is where the first set of articles were found.

However, according to Singer, that volume only covers studies through

1969. From that date on then we are on our own. All major journals

in political science were searched beginning in 1970, including American

Political Science Review, American Journal of Political Science, Journal

of Conflict Resolution, International Studies Quarterly, Journal of

Peace Science, Journal of Peace Research, World Politics, Journal of

Politics, Western Political Quarterly and Peace Research Society

International (papers). In addition, bibliographies in articles found

in these journals were also checked. Needless to say, this does not

represent a complete search -- interesting and relevant articles have

undoubtably appeared in journals in economics, psychology, sociology --

but it does reflect my time and resource limitations. It is also the

case that unpublished papers could not, for obvious reasons, be systematically

covered. My principal concern in this regard are the numerous reports

that have been produced by the COW project which undoubtably contain

considerable additional material of importance. My aim is to incorporate

at least some of these in a subsequent draft.

Having delimited the subject matter and the relevant pieces of

evidence, I was ready to begi... Initially it seemed that the problem

was sufficiently well specified so that the analysis would be straight-
forward. So I began to compile comparative tables across different
studies. But as I developed table after table, an increasing sense of
panic took hold. The studies were so diverse in terms of the variables
analyzed, the types of analyses performed, the criteria (r^2 or significance)
used for judgment that I felt swallowed by the morass of detail -- how
could reasonable comparisons be made between such different operational
measures of prediction variables, between path analyses and bivariate
correlations, between nonsignificant correlations of .7 with an N = 10
and significant r's of .2 based on 100 observations? How could anyone
see a pattern across such diversity?

Following a miserable night in which I dreamed of dancing correlation
coefficients being chased by twisting, turning factor structures,
I realized that I was approaching the problem incorrectly. What I had
before me were the bits and pieces of a puzzle, but I wasn't treating
them like a puzzle. Rather than try to see how the parts fit together,
I was essentially classifying and comparing the pieces. This couldn't
lead to the construction of a final picture. Thinking of the problem
as a puzzle, what one had to do was to begin with one or two pieces and
search among the remaining pieces to find something that "went with it."
In short, one began with one piece -- usually something obvious -- and
then built out from there picking each piece on the basis of its similarity,
or correspondence with previously examined pieces. In effect, such an
approach would not be too different from looking at the problem as a
detective story -- something has happened that you want to account for
and in the immediate environment one discovers various clues. By combining
clues, certain sequences of events are ruled out and others become more

plausible.

The analogy of the puzzle was also useful in suggesting a potential problem. Although I had been careful to define the relevant studies, there was no reason why I should assume that what I had before me were the pieces of a single puzzle and, if indeed they represented more than a single puzzle, it might not always be obvious which pieces went with which puzzles. Furthermore, it was also important to bear in mind that even with all the empirical work that has been done in this area, the chances are high that for any single puzzle not all or even most of the pieces are currently available. So what follows is in effect a mystery story. To the extent to which not all pieces are available, it must be the case that this reconstruction represents a very personal interpretation. However, if the exercise is of any value at all, it should suggest how we might subsequently go about testing this interpretation.

Beginning the Puzzle - Selection of the First Piece

But where do we begin? First, it seemed important to initially separate the pieces into two sets on the presumption that there were possibly two puzzles being examined: a nation-state and a systemic puzzle. While it might be the case that all the pieces fit into a single puzzle, it would probably be easier to proceed with only one set of pieces. Thus I have chosen in this paper to concentrate on the nation-state and will leave to a subsequent draft the further inclusion of the systemic studies. But what is the initial piece within the nation-state pieces that should be used as the starting point for reconstruction? If the analogy with the puzzle contains any validity, it suggests that the choice of an initial piece is irrelevant -- regardless of where one

begins, the final picture should be the same, with one possible exception. If one begins with a piece for which no connecting pieces currently exist, the choice is obviously not very useful.

So with these points in mind, I went through the various studies that had been covered to find what appeared like an interesting, intriguing observation. My choice fell on an observation made by Singer and Small (1972): "most of the war in the system has been accounted for by a small fraction of nations" (page 287). To substantiate this claim, the authors list the frequency of wars for those states most involved in the period between 1816-1965: France and England with 19, Turkey with 17, Russia with 15, Sardinia with 12, Spain with 9, etc. And not surprisingly, most of these same states also account for the greatest number of battle deaths and were involved in wars for the longest periods of time. This observation is completely consistent, though entirely identical, with Richardson's (1960a) indication of those nations most involved in wars since 1815. The difference between the reports lies in differences of definition of war and nation-states, sources used and time periods covered. But for our purposes, these differences are not of major consequence. What is of importance is that both writers agree that a few states have been in most wars. An imaginative analysis of this issue by Bremer (1975) further confirms this observation.

However, we must not leap to the conclusion that international violence is perpetrated by only a select few nations -- what might be termed the "bad seed" theory of war. For the Singer-Small and Richardson observations must be tempered with three other observations. First, in carefully searching through his war data from 1820 to 1939 Richardson discovers that there was only one nation, Sweden, that did not engage in

any international violence. Second, this observation is substantiated
by a very different analysis provided by Naroll (1969). Examining 2,000
years of history to see if he could discern what variables made wars
more or less likely, Naroll concludes that "peace loving nations (defined
in terms of whether they adopted a defensive or aggressive stance) are
no less likely to be involved in war than war-like nations" (pg. 152).
Third, Richardson divides the historical period from 1820 to 1939 into
six 20-year periods and in each period counts the number of new belligerence
that participate in wars. By dividing the number of new belligerence
by the total number of belligerence in each period, he obtains a proportion
that he can now observe from period to period. Intriguingly this proportion
does not dwindle to zero, as would have to be the case if the "bad seed"
theory were correct -- i.e., once the bad seeds were counted in the
first period they would drop out of the numerator for the next period
and thus one would expect the fraction to go to zero. On the contrary,
however, the proportions stay about the same (after an initial high value
due to the first observation period where all belligerence are new):
.73, .37, .42, .34, .24, .37!

If we combine these latter points with the initial observation made
by both Singer and Small and Richardson, it would seem that we must arrive
at the following general observation. International violence is a
widespread phenomenon not confined to just a few states; at one time or
another almost all states have engaged in this type of activity. However,
some nations seem more prone to engaging in this type of behavior than
others. This would seem to be the first piece of the puzzle: why are
some states more war prone?

A possible answer ` this query is that there is a characteristic or attribute possessed by nations which make a nation war prone. While all nations have this quality, some have more of it than others. But if we adopt this line of reasoning, then we must bear in mind the previously cited Singer-Small and Richardson observations. These observations had an implicit time dimension: it was not just that some nations were engaged in more wars than others, it was through 150 year history that some nations were involved in more wars. Furthermore, we can turn Richardson's "new belligerent" analysis around and note that not only does this ratio not dwindle to zero over the six periods, but it also does not increase to one. This suggests that not only the numerator, but also the denominator is remaining roughly the sa e. Thus, if there are attributes, we should probably begin our consideration with attributes that do not change dramatically with time.

Fitting Other Pieces - The Search for the Missing Attribute

One of the most extensive studies of the attributes of nations was done by Rummel (1968). Looking cross sectionally at all states in the mid 1950s Rummel correlates 235 different attributes of nations with 13 measures of foreign conflict behavior. One of these foreign conflict measures is the frequency of war. What is striking about these results is the almost total absence of any relationship (i.e., r is less than .3 and not significant) between what one might consider to be major potential determinants of war-like behavior. For example, Rummel uses a large number of variables to tap such basic concepts as demography, economics, geography, culture, political system, values. With very few somewhat strange and not easily interpreted exeptions -- e.g. number

of marriages per population, length of railroad tracks, and number of

foreign students in country do correlate significantly, $r \geq .3$, with

wars -- there is no covariation. If we consider further, the variable

"military acts," which by Rummel's definition (1963, page 27) reflect

international violence at a somewhat lower level than war frequency, we

obtain a very comparable result: there are still no variables within

the major categories cited above that produce correlations that are at

least .3 (i.e., explain a minimum of 9% of the variance) and statistically

significant at the .05 level. One interesting exception to this is the

number of Mohammedans per population which correlates .38 and is statistically

significant. Finally, one additional measure of international violence

might be considered, Rummel's variable "number killed in foreign violence."

The difficulty with this variable is that it includes the number killed

on all sides of a conflict and is thus not a measure of one nation's level

of international violence. Nevertheless, this variable confirms the

previous results: with the few, essentially same strange exceptions, no

significant correlations of .3 or larger appear with any of the main

attribute variables.

Rummel's study is, of course, confined in time since it covers only

a few years and is thus necessarily biased by the particular events and

characteristics of the mid-1950s. But we cannot easily dismiss the

results on these grounds for they are confirmed by several other kinds

of analyses. In an earlier study, Rummel (1964) provides a series of

multiple correlations between the foreign conflict variables and a series

of dimensions of nations previously obtained in a study done by Berry.

Berry's study was a factor analysis of a large number of attributes of

nations from which he obtained four main dimensions, technology, demography,

size and income. It is the factor scores on these four dimensions that
Rummel correlates (together with several variables from his own work, which
will be discussed later) with the same three variables of war, military
action and number of foreign killed (among others). Once again, the
partial correlations between "war" and any of these dimensions is extremely
low and not statistically significant. "Military action" and "number
killed in foreign violence" each produce one partial correlation that is
statistically significant but it is small (-.26 and -.25 respectively)
and thus cannot be considered too seriously as it explains less than
9% of the variance.

A very different study is provided by Haas (1968). Analyzing 10
essentially European countries (Australia, Finland, France, Germany,
Great Britain, Japan, Norway, Spain, Switzerland, and the United States)
from 1900 to 1960, Haas correlated war frequency with level of industrialization
as measured by per capita productions of electricity. These correlations
were done by state through time but none of the correlations were
statistically significant. When this measure was compared with a scale
of a state's war aggressiveness (measured by experts asked to rate the
states) the results were still not significant.

A third study done by Ray (1974) provides additional support.
Using 10 states similar to those used by Haas (Great Britain, France, Spain,
Germany, Australia, Russia, Turkey, Italy, Poland and Rumania) for the years
between 1816 and 1970, Ray examines the relationship between "status
inconsistency" and several measures of war involvement. "Status
inconsistency" is measured by an index which compares a nation's
capabilities with the diplomatic importance accorded it by other nations.
A nation's capabilities are measured by a combination of such variables

as population, iron and steel production, military expenditures, etc.
while a nation's diplomatic status is determined by the percentage of
states that send diplomatic missions. Thus "status inconsistency" is
a somewhat different attribute measure but not completely unrelated to
those seen above, since it not only includes a state's resources but
also reflects its international position. Although Ray's measure of
war involvement differs from the variable "war frequency"
seen in the previous studies -- Ray considers the number of months a
nation was involved in a war and the number of deaths it suffered --
his results are nevertheless consistent with what has been seen so far.
Correlating through time for each of the nations considered, he finds
that there is no relationship between status inconsistency and war
involvement.

Although both the Haas and Ray studies do raise questions about
the validity of simple Pearsonian correlations when applied to time
series studies (these analyses were done before most political scientists
were aware of the difference between correlations done across
sectionally and through time), the correspondence between these results
and those found in the two Rummel studies seems to suggest that if these
latter analyses were redone with proper corrections, the results would
probably not be dramatically different.

So where are we? Having followed what appeared to be a lead in
Singer-Small and Richardson, we began for a search of possible attributes
that might account for the fact that some nations were involved in more
wars than others. Our initial query was with respect to variables that
one might argue change somewhat slowly with respect to time, thus allowing
us to account for the fact that those nations that get involved in many wars,

do so consistently through time. But the pieces of the puzzle that we have available at this time suggest that this lead was wrong. We have yet to find any meaningful relationship between major types of variables like size or development and war proneness. This, of course, does not mean that such does not exist, it simply means that we do not currently have any more evidence along these lines.

So let's reconsider one of our leads. Suppose the missing attribute does not change slowly with time. There are still conditions under which the Singer-Small and Richardson observations would hold. For example, if high values of the missing attribute produce war proneness, then it might be the case that those nations which were observed to be most frequently in wars, have the greatest degree of oscillation of this attribute, i.e., it fluctuates up and down frequently thus making these nations engage in many wars.

Obviously the attributes which we have chosen to consider as being more likely to change rapidly with time than those described previously, represent a subjective assesssment and some might argue that they belong in the previous discussion. But let us see where this takes us. We return again to a Rummel study (1963, also contained in 1968). The 1963 study was initially an attempt to compare measures of domestic conflict like strikes, government crises, riots, etc. with measures of foreign conflict behavior including, as we saw above, at least three variables that tap different aspects of international violence involvement. After factor analyzing domestic conflict and foreign conflict variables separately to demonstrate that these two sets of variables do contain high inter-correlations, Rummel then factors all measures of conflict together and finds that there is no relationship between the domestic conflict variables

and foreign conflict variables. And indeed if we look more carefully at the bivariate correlations between the 9 measures of domestic conflict and war, we find that there are no correlations above .3 and none that are statistically significant. When we further consider "military acts" and "foreign killed," we pick up one correlation in the .3 range between "purges" and these two variables. Finally, when Rummel correlates the three domestic factor dimensions with the dimension containing the three main international violence variables (the "war" dimension), he finds a small multiple r of .26. Thus with the one intriguing exception of purges, this study suggests that measures of internal disruption, which we are here defining as attributes which fluctuate through time, do not appear to predict the international violence behavior of states.

These results were confirmed further by a replication study done by Tanter (1966). While Rummel's study was for the years 1955-1957 (values of the variables were collapsed over the three years for a given state) for 77 nations, Tanter considered the period from 1958-1960 for the existing 83 nations of that period. The factor analytic results were roughly comparable to those obtained by Rummel and, more pointedly, the correlations between each of the internal conflict measures and the frequency of war variable were again essentially nonexistent. Correlations with "military action" were also near zero but two correlations in the .3 range did appear between both "assassinations" and "government crises" and "foreign killed." Finally, a multiple regression of several of the main domestic conflict variables and the war dimension failed to produce a correlation above .3.

Collins (1973) provides additional evidence along these lines. In a study very comparable to the ones done by Rummel and Tanter, Collins

analyzes 33 independent African states between 1963 and 1965 (also
at one point collapsing values over time). While his variables roughly
correspond to ones used by Rummel and Tanter, due to the types of
countries being examined and the sources used, slightly different
measures of domestic conflict were used. Furthermore, the authors argue
that factor analysis of these variables produced meaningless factors
and so they adopted to group variables on intuitive grounds. Thus
variables were combined to produce seven main domestic conflict variables
(e.g. riots, strikes, political clashes were combined into one variable).
Also, probably because these countries were involved in almost no wars
during this period, the international violence measures include only
"military violence" and "number killed." Nevertheless, the results
are largely the same. Measures of political clashes, subversion, elite
instability, and political arrests were not correlated with either
measure of international violence involvement. There were, however, two
interesting exceptions to this absence of correlation: revolutions
did significantly correlate with "military violence" at .39 and domestic
suppression did correlate significantly with number killed, r = .36.

Haas' study, discussed previously, provides further support, though
here one might question my categorization of these variables as
"fluctuating." In any case, whether one considers these results here
or in the earlier discussion, the results are equally nonexistent.
Correlations between war frequency (again done through time by state)
or the war aggressiveness scale with unemployment rates, suicides,
homicides, and alchoholism are all extremely low.

Finally, in a somewhat different type of analysis, Zinnes and Wilkenfeld (1971) also suggest that domestic conflict is not related to international violence. Using the results of a factor analysis of both the Rummel and Tanter data, a series of transition matrices were constructed between levels of domestic conflict as measured by the two factors of internal war and turmoil and the levels of foreign conflict behavior as measured by the dimensions of war, belligerancy and diplomacy. For our purposes only the war dimension is relevant. By holding the transition between amount of war constant, e.g. considering only transitions between no war and some war in a subsequent time period, the effects on this transition of the two domestic conflict dimensions could be examined. It was found that the domestic conflict dimensions did not influence the transitions on the war dimension.

In short, the evidence thus far would seem to imply that we have once again come up against a dead end. But not entirely. There are a series of studies that at least initially seem to contradict the above results but, when considered more carefully, in fact suggest yet another line of attack. Provoked by the Rummel and Tanter results, Wilkenfeld (1968, 1973) redid these analyses over groups of states categorized essentially by government type. He found that by so reclassifying the states, some significant positive correlations could be found between domestic conflict and foreign conflict. For our purposes the important relationships are those correlations between each of the domestic conflict factors and the war dimension. Using factor scores for the nations Wilkenfeld finds that the "revolutionary" variable correlates significantly, $r = .55$, with the "war" variable for centrist countries and that the "turmoil" variable correlates significantly with the "war" variable for

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polyarchic nations, r = .39. In short, if we take into account something about the structure of governments, then it would appear that certain types of internal disruption might predict the international violence behavior of a nation.

Another intriguing result in this regard is found in Hazelwood (1973). This study provides a considerably more complex statistical analysis than has been seen thus far, but by examining what are essentially the regression coefficients, we can discern at least one result of relevance. In his first canonical correlation, Hazelwood finds that population diversity and ethnic diversity together with Rummel's old dimension of turmoil relate very heavily with Rummel's war dimension. This is particularly interesting if we recall one tiny piece of evidence seen earlier: in the 1968 Rummel study a significant correlation of .38 was found between proportion of the population that was Mohammedan and "military action." If the independent variable in Rummel's analysis is thought of as a possible measure of population diversity, it would be consistent with the Hazelwood results. Thus we see, as in the Wilkenfeld study, that when several variables are combined, some relationships do appear.

A third study of interest is one done by Bobrow, et. al. (1973). Although Bobrow's principal interest is the impact of military assistance, he reports one analysis that is relevant in this context. Looking at 15 Asian nations from 1955-1966, these researchers correlate within nations and over time a variable denoted "political strife" with another variable labeled the "international cooperation/conflict ratio." "Political strife" is a variable composed of demands, instability and domestic violence and thus overlaps the domestic conflict variables

seen so far but clearly includes additional factors. Although the cooperation/conflict ratio is not really a direct measure of international violence, one can argue that it is an indirect measure since conflict includes violent behaviors. In the last series of analyses reported in this paper, one finds that the r's between "political strife" and the cooperation/conflict ratio are very high for seven of the countries, some correlations exceeding .8.

If we compare these three studies, we see that each suggests that a relationship exists between variables that measure internal disruption and variables that measure international violence behavior when the internal conflict measures are taken together with other attributes of nations, governmental structure (Wilkenfeld), population diversity (Hazelwood), or demands and instability (Bobrow et al.). Perhaps it is the case, looking back at our first line of attack, that our initial lead took us nowhere because we were looking only at these more time-stable variables in the absence of a consideration of factors that fluctuate more readily. Thus governmental structure or population diversity, as more stable attributes of nations, have to be combined with variables like internal disruption before we are able to predict whether a nation is war prone.

Although considerably more difficult to compare because the analyses are so different, the results reported by Choucri and North (1975) appear to provide yet another link. Looking at the major countries (Britain, France, Germany, Italy, Russia, Austria) involved in the First World War, these researchers examined the 35 year period from 1871 to 1914. They present a model composed of a series of simultaneous equations which link a variety of variables, through various paths, to violent behavior.

This system of simultaneous equations is fit to each of the six countries separately. Since the results do in fact differ from case to case, it is somewhat difficult to draw general conclusions. Furthermore, violence is not directly linked in the model to variables comparable to those seen in the other three studies. Nevertheless, there are cases in which the variables "population density" and "national income per capita" do affect violence indirectly by affecting colonial area, military expenditures and alliances. Thus while Rummel found no relationships between measures like population density and international violence, Choucri and North suggest such links might exist if indirect relationships were postulated and examined through time.

Another Perspective

Let us move now from this corner of the puzzle to a different corner. Another intriguing observation about international violence is the consistent correlation found between defense expenditures and international violence. Choucri and North (1975) find this relationship both directly and indirectly through the medium of alliances for almost all of their six cases. In Rummel's study of the 235 variables (1968) a variety of indicators of defense expenditures (Defense Expenditures, defense expenditures relative to population, government expenditures and GNP, and number in military) all produced significant correlations in the range of .35 with at least one of the three violence variables. Similarly, Naroll's (1969) study of 2,000 years of history allows him to conclude that "armament tends to make war more likely," and Richardson's analyses of the cost of defense per population and the number killed in wars, produced a significant, though rather low correlation. Further, Weede (1970) finds a significant correlation of .47 over 59 nations (1955-60) between a variable combining military personnel

per population and defense expenditures as a proportion of GNP and violent foreign conflict. Sylvan (1976) examined 15 Asian countries between 1956 and 1970 using a quasi-experimental design. Countries were divided into experimental and control groups on the basis of the amount of military aid received relative to the military expenditures of that country. The experimental group consisted of those nations in which this ratio exceeded one, the control group was composed of the remaining nations where the ratio was less than one. Sylvan constructs an index that compares a variety of cooperative actions with conflictful behaviors, where one component of the conflict behavior is international violence. Plotting this ratio through time, he is able to show that following military assistance, the experimental group evidences a sharp increase in its conflictful behavior when contrasted with the control group. Finally, Newcombe (1969, 1973) suggests that a relationship exists between a "tension" ratio and the frequency of war. The tension ratio is constructed by comparing actual defense expenditures with "predicted," where predicted defense expenditures are determined from a regression of defense expenditures on GNP. Thus the tension ratio indicates whether a nation is "over" or "under" defensive when compared to all nations in the system.

In short, we have a set of results over a variety of different time periods and nations that consistently indicate, contrary to deterrence theory notions, that more arms increase the violent behavior of nations. But what do these results say with respect to our previous analysis? With respect to our previous categorizations of slow and fast changing variables, one might argue that defense expenditures fit the latter category. Defense expenditures is a variable which is clearly less like the variable

geographical size and more like the variable internal disruption. In part this is correct, yet it seems a mistake to place defense expenditures in the same set of variables as indices that measure internal disruption. Unlike the other variables that have been examined, defense expenditures are typically responses to environmental conditions. Nations usually arm not as a function of internal problems (though there are obviously some conditions under which this might happen) but in response to what is happening externally. Indeed, recent studies of arms races largely support this supposition (see Rattinger, 1975, 1976; Hollist, 1977a, 1977b; Zinnes and Gillespie, 1973). Consequently, the relationship between defense expenditure indices and international violence could be seen as another clue: perhaps we have focused too narrowly on the attributes of nations and have ignored too long the environmental conditions that surround nations. Those countries that were found to be most frequently involved in wars could be situated in very special environments. Perhaps the question is not what attributes make a nation violence-prone, but what environmental circumstances provoke violent behavior.

There is another piece of evidence that directs our attention to the environment. The most striking results in the Rummel (1963) and Tanter (1966) analyses are the high significant correlations, in both data sets, between measures of foreign conflict behavior. Thus both Rummel and Tanter find high significant correlations between such measures as threats, accusations, protests, severence of diplomatic relations and the three measures of international violence (r's in the .6 range). This suggests that when nations react with violent behavior, such reactions are accompanied by a variety of other forms of hostile behavior which also reflect environmental provocation. Since it seems

unlikely that a nation would threaten or protest in the absence of
something occuring in the external environment, the implication is strong
that violence is a reaction, a response to external stimulii.

One might feel that the Rummel-Tanter foreign conflict measures
are simply indicators of behavior directed externally. Thus the fact
that other measures of foreign conflict behavior correlate with frequency
of war or military actions might simply be an indication that relationships
exist among foreign policy behaviors. But this is not the case as can
be seen in a study by Terrell (1972). Examining 75 countries for the
period of 1955-1960, Terrell factor analyses 18 variables which measure
different facets of international involvement and obtains four factors
which roughly correspond to political (number of embassies in other
countries, number of treaties, number of IO memberships, number of
representatives at UN, etc.), economic (exports/GNP, trade/GNP, etc.),
social (visitors/pop., foreign mail/pop., etc.) and military (number
military treaties/total treaties, military aid from US, etc.) involvement.
Correlating factor scores on these four dimensions with the Rummel
"war" dimension, Terrell finds that none of the correlations are greater
than .28 though the economic and social factors do produce significant
negative correlations. Thus it would appear that a difference does exist
between those behavioral variables that measure a form of interaction
with the environment and other variables that might be considered as
measuring externally directed behavior (e.g. the number of embassies,
or representatives at the UN).

Thus we have two pieces of evidence: correlations between
international violence and (1) defense expenditures and (2) other forms
of hostile behavior. Are there other indications that international

violence is a function of external stimulii? Surprisingly, there are

rather few empirical studies of hostile interactions involving violence.

However, there is one of direct relevance and two others which shed some

light in this direction.

Although Milstein's (1972) principal interest was in tracing the

impact of American and Soviet influence on the Arab-Israeli conflict,

his initial analyses provide us with some important information about

interaction patterns with respect to violence. Milstein examines the

period between 1948 and 1969, and through a content analysis of newspapers,

counts the number of weeks each side engages in various types of violent

actions (encounters between government forces, encounters between guerilla

forces, attacks on civilians, attacks on installations, mobilizations,

troop movements, declarations of emergences, alerts). He then correlates

the activities of each of the Arab countries (Egypt, Jordan, Iraq, Saudi

Arabia, Syria and Lebanon) with Israel within each action type and for

different lagged conditions. For example, he correlates Egypt's attacks

on civilians with Israeli's attacks on civilians. Not all pairs of

countries for all action types produce high correlations but government

force encounters and guerilla encounters produce consistently high r's

(between .56 and .82) between Egypt and Israeli and Jordan and Israel.

The evidence from the other studies is less direct, though they do

imply that violence is at least in part a function of interactions between

nations. Weede (1970) correlates contiguity in a dyad and violent foreign

conflict between the members of that dyad for 59 nations in 1955-60. He

obtains a significant correlation of .48. Thus violence is a function of

contact and, by implication, of interaction. In the second study Richardson

constructs a histogram which shows how many dyads had how many years of

peace. Inspection of this histogram shows it to be monotonically decreasing, almost as if it could be described by a geometric progression. Richardson concludes from this histogram: "This decreasing frequency of retaliations . . . as the interval of peace increased is what we should expect if a slow process of forgetting and forgiving went on" (SDQ, pg. 200). Or to state it somewhat differently: the longer peace exists between two enemies, the less likely they will fight each other again.

In a third analysis, Richardson compares allies and enemies across the two world wars and finds that if two nations fought against one another in the first war, they were more likely to fight each other in the second. This latter result was also confirmed by Starr (1974) in a more extended analysis of the changes between friendship and enmity across successive wars: those who were enemies before had a greater probability of being enemies in a subsequent war.

Thus the environment, and more specifically the inputs and provocations from the environment, appear to be important in shaping and determining the violent behavior of nations. But to this point, we have been considering the environment in a very special sense as a stimulus or pin prick that forces a nation to respond. However, the environment could shape the behavior of a nation in a more passive way. Are there any studies which examine the impact of external environmental conditions on violent behavior?

Another small analysis by Richardson is relevant. Richardson correlated the number of wars with the number of borders for 33 states and obtained a statistically significant $r = .77$. While one might argue that borders, like geographical size or GNP, are an attribute of a state, it is clear that unlike the variables considered earlier such as population,

square miles, GNP (Rummel, 1968), borders imply a direct contact with
the external environment. Thus the implication of this study is that
the greater the amount of contact with the external environment, where
"amount" means number of other states, the greater the likelihood that
the state will engage in violent behavior.

There are two additional studies that develop further the inference
that external conditions shape the violent behavior of a nation. Weede
(1976) focuses specifically on Asian countries for the period between
1950-69. By forming all possible dyads between these countries, he
separates out those dyads that are contiguous from those that are not and
adds to the contiguous dyads all other contiguous dyads involving one Asian and
one non-Asian country. Weede's interest, however, is not simply contiguity,
but rather the power relationship between contiguous dyads: when a
dyad is contiguous and one has overwhelming power, is this dyad more
or less likely to become involved in a war when compared with a contiguous
dyad in which the two nations are more equal in power? Defining power
preponderance as ten times the GNP of another nation, Weede shows in
a variety of analyses that war is considerably less likely in the presence
of preponderant power.

These results are further supported by a very similar study done by
Garnham (1976). Although the basic question posed by Garnham is almost
identical to that posed by Weede, the research design is quite different.
Garnham proceeds to select his cases . analysis by considering all
states that experienced lethal international violence during the period
1969-73. He finds 16 conflicts involving 24 nations. For each of the
24 nations he identifies all contiguous states. Thus he has in effect
16 cases of contiguous states that engaged in violence and by examining

the borders, he finds 62 contiguous dyads which did not engage in violence. The question then is whether a difference can be found between these two sets of dyads on the basis of power differences. Garnham proposes six different variables to measure power: geographical area, population, GNP, KWH, military manpower and defense expenditures. Using both pattern recognition and discriminate function analysis, he finds that "lethal conflict is more probable between continuous nation states of approximately equal national power."

Thus these two studies not only confirm further the importance of the external environment, they imply additionally that it is not simply contact or amount of contact that is the important ingredient, but that one must also consider the quality of that contact. If a tiny nation is surrounded by five large nations, i.e., shares frontiers with five other countries, the Richardson analysis would suggest that this nation has a high propensity to go to war. But the Weede and Garnham studies temper this result to suggest that the relative powers of the contiguous neighbors will make this tiny nation less prone to violence. Of course, the interesting and as yet unaswered question is how these variables interact to shape violent conflict: what is the interrelationship between frontiers, contiguity, power and international violence?

Conclusion: Is there a Picture?

This brings us to the end of our construction of the puzzle, not because we have any final picture, but because we have run out of pieces. Undoubtably, there are pieces that I have missed which should be added, but hopefully the above construction represents a large proportion of what currently exists in the published literature. So what can we say on the basis of this partially completed picture and what kind of answer

can we give to Rapoport?

The conclusions are not extensive and in one sense they could be considered both obvious and trivial. The partially completed puzzle suggests that there probably does not exist any single obvious attribute that makes a nation war prone; such attributes as governmental structure, level of development, amount of resources do not make a nation more likely to engage in international violence. Second, it would appear that the internal problems which a state must face do not make a state either more or less war prone; unemployment, civil strife, suicide rates are not keys to international violence. On the other hand, there is an indication that combining the first and second types of variables does begin to point in the direction of discriminating between those nations that become heavily involved in war and those that do not. If we combine measures of internal strife with such factors as governmental structure and population diversity, we begin to be able to predict international violence. Thus a third conclusion is that there are probably very special combinations of national attributes with internal problems that produce aggressive foreign policy behavior.

A fourth conclusion is that we must not become obsessed with the attribute focus. We must realize that nations react to inputs from an external environment. Analyses of defense expenditures and other forms of hostile foreign behavior, clearly indicate that the violent behavior of a nation is related to the stimulii it receives. And this is reinforced by the one study that has analyzed the violent interactions of nations in the middle east conflict. Finally, we must think more generally of the external conditions that surround a nation, conditions which either constrain or make more plausible the use of warfare. Contiguity and

power relationships are clearly such conditions and the results thus far suggest that these have a definite impact on violence.

Our conclusions are far from earth shaking. The mystery is a long way from being solved. Indeed, we seem to have two very separate parts of a puzzle that do not obviously seem to fit together -- attributes and environment. Thus in one sense we have not been successful in answering Rapoport's challenge. We clearly cannot say that there is overwhelming evidence to suggest that x and y predict z. On the other hand, the situation is not as bleak as Rapoport and others might lead us believe. Perhaps the most encouraging conclusion of this review is that it _is_ possible to construct at least a partial picture -- a number of the pieces can be shown to fit together. Thus it is not the case that we are each working in a vacuum producing studies that have no relationship to one another. Nor is it the case that the evidence is contradictory. Indeed it is intriguing and encouraging to discover that even though some of the studies contain statistical difficulties, it is, nevertheless, possible to see an overall reinforcement across results.

My answer to Rapoport then is that while we surely do not have laws, the bits and pieces that have been produced and are being produced seem, at least to this viewer, to be slowly moving us in this direction.

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